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This paper analyzes and evaluates the methodological approach developed by B. G. Glaser and A. L. Strauss in THE DISCOVERY OF GROUNDED THEORY (Chicago: Aldine, 1967). Smith and Pohland's major intent is to raise Glaser and Strauss' most significant concepts and issues, analyze them in the context of seven of their own studies, and in conclusion lead to a formal methodological strategy for their current project, a study of the utilization of Computer Assisted Instruction in Eastern Kentucky. Introductory sections describe the process of generating "grounded theory" (which unlike the preeminent logico-deductive process is "an initial, systematic discovery of the theory from the data of social research") and explain Smith and Pohland's greater emphasis on developing a thorough descriptive account or narrative in their conception and use of nonparticipant observer methodology. The major sections of the paper are (1) "Theoretical Sampling in the Development of Grounded Theory" which includes discussion of "theoretical saturation," "slices of data," "comparative analysis of groups," "theoretical sensitivity and insight"; and (2) "The Discovery-Verificational Continuum Reanalyzed" which defines the "constant comparative method of qualitative analysis," analyzes each step in light of five field work examples, and isolates major differences in approach. A concluding section discusses application of grounded theory. (JS)

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GROUNDED THEORY AND EDUCATIONAL ETHNOGRAPHY: A METHODOLOGICAL ANALYSIS AND CRITIQUE

by

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and
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GROUNDED THEORY AND EDUCATIONAL ETHNOGRAPHY: A METHODOLOGICAL ANALYSIS AND CRITIQUE[1,2]

by

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INTRODUCTION

This particular paper analyzes and evaluates the methodological approach developed in *The Discovery of Grounded Theory* (Glaser and Strauss, 1967). In a sense, the discussion might well be considered a review of their book in the context of our several attempts to use a similar approach (Connor and Smith, 1967; Pohland, 1968; Pohland and Gussner, 1968; Smith and Brock, 1969; Smith and Geoffrey, 1968; Smith and Keith, 1967, 1970). The intent will be to raise their most significant concepts and issues, and to analyze them in the context of our earlier work and, in conclusion, lead to a formal methodological strategy for our current project, a study of the utilization of Computer Assisted Instruction in Eastern Kentucky (Smith and Pohland, 1969).

If one takes the position as Glaser and Strauss do that an "ideal" theory is one that: (1) allows prediction and explanation of behavior; (2) is useful in theoretical advance; (3) is usable in practical applications; (4) provides a stance to be taken toward data; and (5) guides and provides a style for research on particular areas of behavior [p. 3] as well as the more formal requirements of logical consistency, clarity, parsimony, density, scope, and integration [p. 5], then one is faced with the basic problem of how one goes about generating theory consistent with these criteria. Historically, the logico-deductive process has been preeminent. As an alternative, Glaser and Strauss propose "as the best approach an initial, systematic discovery of the theory from the data of social research [p. 3]," or, more simply, "grounded theory."

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[1]This discussion will appear as Appendix I of Smith, L. M., & Pohland, P. A. *Arithmetic in Appalachia*, 1969, an account of an innovative computer assisted instruction program.

[2]Prepared for the AERA presession, *Anthropological Methods in Educational Research*, February 1969, Los Angeles, California.

The basic premise that the authors work from is that *"generating a theory involves a process of research* [p. 6]." While this admittedly results in a product phenomenologically, rather than logically, derived, Glaser and Strauss propose that such theory has as a minimum two cardinal virtues: (1) "theory based on data can usually not be completely refuted by more data or replaced by another theory [p. 4]," *i.e.*, permanence and (2) "grounded theory can help to forestall the opportunistic use of theories that have dubious fit and working capacity [p. 4]." Our position is much the same. In a very basic sense this is precisely what we have been doing with our projects. We have accented the twin goals of careful description initially and secondarily in time the development of concepts, hypotheses, and models from the data. Our current Computer Assisted Instruction project is a case in point. With the exception of in situ "interpretive asides" and some speculative "summary observations and interpretations," our data file is comprised largely of careful descriptive material: classroom instruction, children working at the teletypes, computer printouts, system breakdowns, training sessions, the social milieu, formal documents, and the like. Conceptual development, hypotheses formation, and model building while embryonic are goals for later efforts.

Contrasting Emphasis on the Narrative

A contrast in our conception and use of nonparticipant observer methodology and that of Glaser and Strauss focuses on our greater concern for developigg a thorough descriptive account or narrative of the settings and the action which takes place within that context. They deprecate that effort along with any effort toward verification. They reason that more than minimal accent on these two processes "stifles" the generation of theory. They comment:

> But when generating is not clearly recognized as the main goal of a given research, it can be quickly killed by the twin critiques of accurate evidence and verified hypotheses . . . . The analyst's confidence is destroyed because everyone involved fails to realize that accurate description and verification are not so crucial when one's purpose is to generate theory. This is especially true because evidence and testing never destroy a theory (of any generality), they only modify it. A theory's only replacement is a better theory [p. 28].

Again:

> First, he [the researcher] must remember that he is an active sampler of theoretically relevant data, *not an ethnographer* trying to get the fullest data on a group, with or without a preplanned research design [p. 58. Italics ours].

Somehow, we are not convinced. Nor are we persuaded that Glaser and Strauss meant entirely what they said. If we interpret them correctly, a careful descriptive account is a prerequisite for "grounded theory," particularly if one's goal is to achieve theoretical credibility and "goodness of fit." To us it seems that deprecating the descriptive account while arguing for grounded theory tends toward a contradiction in terms.

As we have argued in several papers, our own position is quite different. One initial reason centers precisely on the issue of credibility. We developed some of the descriptive material in *The Complexities* explicitly for this purpose. Second, we have some strong feelings and beliefs that the utilization of theory for the solution of practical problems in education is very important. We think this requires a fairly intensive descriptive account, particularly since in education teachers and administrators tend to think in situationally specific terms. One needs to know the context out of which the concepts came and to which they will be referred back. Third, the kind of theory that we have been generating is more of what Glaser and Strauss would call a substantive rather than a formal theory. In this sense it is more closely tied to a particular setting and the requisite description of that setting. Fourth, it seems to us that if one is to use the "constant comparative method" that Glaser and Strauss recommend and to which we generally ascribe, one needs a rather thorough data base from which to proceed. We would argue that this is particularly essential when, as in our current study, more than one investigator is involved and the work is carried on in multiple settings. Fifth, we take the position that when an investigator begins his work, he does not know the full range of theoretically relevant concepts. In a study such as our CAI project the dynamics of the innovation change over time, and a concept of theoretic relevance may be only dimly perceived or perceived not at all at the beginning. Again, this suggests the necessity for a detailed descriptive account. Given the choice of an over-abundance of data containing much chaff but a potentially dense data base, or a choice of little chaff but a potentially thin data base and a resulting "thin unvalenced theory," we opt for the former. Sixth, and related to the preceding, is the possibility of integrating data obtained from one study with that of other studies. We would simply maintain that the richer the descriptive account of each study, the easier and potentially more fruitful this cumulative effort can become. Seventh, most of the teaching in which we have been involved has been of an applied sort, and the people who finish the training programs go into the kinds of settings that we have been studying. Once again, this provokes a need for a more careful view and descriptive account. Eighth, in working with a number of students and others who have used and/or wanted to learn the method there is often a good bit of anxiety about the way the method works. The descriptive or narrative job, while difficult to write in an interesting and lucid style, is, at least initially, an easier place to begin. Only after one has struggled a bit with the description and begins

to see the possibilities of organizing and abstracting from such concrete materials the broader ideas, concepts, hypotheses, and models can one move freely and well.

In contrast, and to make a minor point, one of our chief objections to much anthropological and historical writing and research lies in the fact that anthropologists and the historians often never get beyond trying to straighten out the narrative, that is, "telling the story." This reiteration of daily life in a variety of inaccessible communities frequently does *not* make an interesting account, for us, and often leaves the materials at a very concrete level. In this form the writer can never answer the "so what?" kind of question regarding the purpose of their efforts.

We have a strong conviction that ultimately all hypotheses and models must be put to careful verification. The usual strategy here becomes the correlational analysis, field study, or even better, the laboratory or field experiment. Additionally, however, the building of a series of interrelated participant observer field studies in which one is basically generating a theory also has, as a side result, the gradual accumulation of propositions that have more than a bit of credibility and that approach the form of principles. We will return to this point later when we talk in more detail about the constant comparative method of qualitative analysis.

Theoretical Sampling in the Development of Grounded Theory

Introduction

Glaser and Strauss define "theoretical sampling" as "the process of data collection for [the purpose of] generating theory whereby the analyst jointly collects, codes, and analyzes his data and decides what data to collect next and where to find them, in order to develop his theory as it emerges [p. 45]." This seems to be the most important conception in their position. It develops further some things that we have started earlier on an intuitive level and need to refine at this point.

For instance, the idea of theoretical sampling formalizes some of our thoughts of having enough evidence, enough data in a particular area, and moving into other related problems. In the Kensington situation (Smith and Keith, 1967) this involved concerns within different pupil age levels or divisions, Independent Study Division versus Transition versus Basic Skills, issues internal to the school, and external phenomena such as the parent council. In the CAI project theoretical sampling involves the preliminary and basic view of the realities of the way CAI is used in a particular school or in several specific schools, as well as the more general issue of how one goes about introducing and implementing an innovative practice in an established school system. It concerns also a fundamental issue in

continuing the elaboration of a psychological theory of instruction. From this point in time, we propose to "see how it goes," and by this we mean to seek relationships to some of the interlocking parts of earlier problems and projects in which we have been involved. "Seeing how it goes" also involves all of the new ideas and issues that occur serendipically as we are in the situation. For instance, there is the interest we have already generated in the politics of education, the unique role of the county school superintendency, and so forth.

In short, Glaser and Strauss's discussion of theoretical sampling seems to be very important. Yet we have experienced a good many difficulties in following the tenor and logic of their argument. The basic difficulty stems from the dependence of their discussion upon the issue of what they call selecting groups, which is related to their earlier point of selecting comparison groups and what they discuss later as the constant comparative method. The confusion lies in that theoretical sampling should, we think, be the basic rationale for a more general theory of research strategy or decision making in the field and not just a rationale for the selection of groups to be studied. In this sense the selection of groups is but one of many problems that the researcher has to decide upon, and hence may have some atypical features when thinking through the issues of theoretical sampling. In summary, we are arguing that their subordinate point, theoretical sampling, should be the superordinate issue. It is essentially from this position that we will analyze theoretical sampling in the context of CAI and our earlier work.

Theoretical Saturation[3]

After the consideration of the several introductory issues we can now move to a more explicit attack on their conception of theoretical sampling. They have three subconceptions that are important in understanding their point of view. These include: theoretical saturation, slice of data, and depth of theoretical sampling.

> *Saturation* means that no additional data are being found whereby the sociologist can develop properties of the category [p. 61].

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[3]Their concept of depth seems comparable to saturation. They make a distinction between "core theoretical categories," that is, those with the most explanatory power which are pursued in great depth ("saturated"), and the more peripheral categories. Since there may be some doubt as to the identity of core categories, Glaser and Strauss recommend continued saturation of all categories until the issue is resolved. The alternatives are either an unwieldy mass of data or "a thin, unvalenced theory."

Once again, they put this point in the context of selecting groups, and this seems to us to be the wrong subordination-superordination of ideas. In our work in the past and in our future work with CAI, the broader context, for which we would argue, is that we begin with an initial array of problems and issues, that is, strategies of teaching, innovation, and so forth. As we work in a particular context or setting we try to exploit that setting for all of the information and all of the ideas that we can find. In a sense, we keep looking until we can generate no more of what we've called in the past "insights" and "interpretive asides." It is at that point that we tend to quit. In this situation our experience has been that beyond the initial focus, the narrative or story line soon carries us into a whole variety of other problems and issues that we had not anticipated in our preliminary entrance to the problem. This is moving from the foreshadowed problems into the scientific issues.

Perhaps here is a further reason for the need for description. When we finally know enough about a particular setting and have described it in detail and there are no more issues coming out, then it is time to quit. Perhaps in this context "really knowing" not only means having it conceptualized but also being able to describe its day to day workings as well as, if not better than, the man who is actually living and working in the setting.

Glaser and Strauss make an important subpoint here as they talk about procedures in the joint collection and analysis of data. This gets them involved in memo writing and discussions. Our own use has been much more the "summary observations and interpretations" kind of documents in which we have been involved. In a sense each of us is talking to himself rather than talking formally to his colleagues. Hopefully, in the CAI we will increase this kind of activity as well as increase the joint development of data and the development of insights within the data reporting as we did on our recording at the time of the summer 1968 CAI workshop. At that time we talked together and then recorded one set of "Summary Observations and Interpretations." This seemed to facilitate recall and interpretation of specific events.

Further, they discuss the determination of saturation as a combination of the empirical limits of the data, the integration and density of the theory, and the analyst's theoretical sensitivity. Their sentence goes as follows:

> The criteria for determining saturation, then, are a combination of the empirical limits of the data, the integration and density of the theory, and the analyst's theoretical sensitivity [p. 62].

Here again, it seems to us that we are back to description, summary observations, and interpretive asides.

This suggests a further reason for careful description. Unless one can tell the practitioner, a person living in the situation, exactly what's going to happen he may see the relevance of some of your concepts and models and yet he would still argue that they are superficial and do not include the totality of life there. Perhaps the additional point that we need lies in the notion of integration and density of the theory. You can't have this, we don't believe, without the intense description or what he was calling earlier "the empirical limits of the data," and what we have called the "data base."

Another aspect of the concern over description as well as generating theory lies in the problem of when to terminate the sampling. As they say, "learning this skill takes time, analysis and flexibility, since making the theoretically sensitive judgment about saturation is never precise. The researcher's judgment becomes confidently clear only toward the close of his joint collection and analysis, when considerable saturation of categories and many groups to the limits of his data has occurred, so that his theory is approaching stable integration and dense development of properties.[p. 64]." In our work the problem is somewhat simplified as this is mainly a function of the rhythm of the educational enterprise and the fact that we are dealing with phenomenally discrete situations, for instance, the beginning of a semester and the end of a semester, or the beginning of a year and the end of a year. Once again, the description of such a period has a kind of integrity which states a beginning and an end. The theoretical quest builds into this.

Slices of Data

An additional theoretical point within methodology occurs in the interrelationship between the description and the genesis of the substantive theory. As we think about the CAI project and our tentative decision to focus upon two specific school situations (Breckinridge School in Morehead and the Paintsville Independent Schools), one of our concerns has been to obtain enough data to give a thoroughgoing description of these two settings and to build our theory out of those. The point we want to make regarding this decision plus the need for description in the genesis of theory is the tremendous amount of time that it takes to become well enough acquainted with a group of people so that they will talk freely about what they do and how they do it. In this sense, one adds to the direct observing the more informal comments and conversations that one obtains in the course of getting to know the people as they work in the setting. To fathom and probe that source of data carefully takes a tremendous amount of time. As one tries to build an awareness which goes into the descriptive account one communicates this kind of interest and orientation to the people in the setting, and these people then tend to speak more readily about their jobs, problems, and ideas. In the past we've found this to be a very rich source of hypotheses about the nature of teaching. In a sense it has been built into most of

our analyses; for instance, teacher plans and teacher strategies are basic conceptions in our theories of teaching. The relevant data would be less available without the self-reported descriptions.

In short, we are making a point comparable to Glaser and Strauss's conception of slices of data, *e.g.*, "Different kinds of data give the analyst different views or vantage points from which to understand a category and to develop its properties . . . [p. 65]." In more general psychology this has been discussed as multitrait and multimethod matrix (Campbell and Fiske, 1959). In one of our uses of participant observational techniques, the teacher apprenticeship at City Teachers College (Connor and Smith, 1967), we were struck by what a test maker might call the validity of his measures. We observed our apprentices teach a variety of lessons. We talked with them informally about their problems, plans, intentions, and practices in these same lessons. We listened to them talk with each other and with their cooperating teachers about the lessons. And finally, we talked informally with the cooperating teachers, principals, and supervisors about the same events. In most instances we got along very well with the various persons; in some instances we were father-confessors who were out of the authority structure, who knew what was going on, who would listen, and who would empathize. This aspect of method has a potency which we are only now coming to appreciate; we think we obtained a valid picture of the apprenticeship. This seems to be what Glaser and Strauss mean by "slices of data."

Comparative Analysis of Groups

The major thrust of their discussion of the strategy of "comparative analysis" seems to focus on the fact that no issue can really be clear until it is presented in the context of other similar and relevant issues. For instance, Glaser and Strauss cite the illustrative case of Louis Wirth's study of the Chicago ghetto and their developing a contrast with European ghettos. It is only in such a comparison, or such a contrast, that one really sees the distinctive features of the new. In our own work we found that we kept turning back to our prior studies. For instance, in the Kensington analysis (Smith and Keith, 1967, 1970) and in the City Teachers College (1967) analysis we kept referring back to situations, ideas, and points of view out of *The Complexities* analysis. In a sense we did this intuitively without trying to formalize the potency of what we were doing as a major method in the study of educational groups and educational settings. At this point, the beginning of the CAI study, we have the potentiality of clearly and directly attacking some recurring issues. For instance, in a number of preliminary statements that we have made there are continuities of the following sort: first, the children in the Washington School were essentially poor, rural whites who had migrated to the city. Hopefully, we will see some of the origins of these children in Eastern Kentucky. Secondly, the CAI program is really a mode of instruction, and we have been involved in other modes of instruction

in both the Washington School and Kensington. Perhaps even more graphically, although not analyzed to this point, are our data and preliminary thoughts on the Suburban High School project (Smith and Brock, 1969) and the issues of discovery learning. Third, CAI is an important educational innovation, and we have been much involved with educational innovations in the Kensington School project. Fourth, we have dealt briefly but not clearly with problems of school-community relations in several of our studies, but we have not focused upon the interrelationship between the community and the school. At the Washington School there were the racial issues in the community, and at the Kensington School there were issues in the parents council, a type of PTA, and the way in which the broader institution of the Milford PUblic Schools affected the Kensington School itself. Our quest for the nature of the milieu and culture of Eastern Kentucky and the beginnings of readings that we've been engaged in, such as the Kluckhohn and Strodtbeck book, *Variations in Value Orientation*, the several accounts of the Elizabethan period in contemporary America, for instance, the Sherman and Henry account of the *Hollow Folk*, the writings of Jesse Stuart, and other recent books, provides part of this as well. Fifth, the issues of elementary versus secondary education and the comparative kinds of elementary education from the several contexts that we have been in also will prove helpful. Sixth, if we are involved in the Breckinridge School as one of our continuing and strongly studied settings, we will be involved in another aspect of teacher education, and here the interrelationships with the City Teachers College study are also very important. In short, we have a number of settings in which we have spent considerable time and which we have been involved in interlocking problems, issues, hypotheses, and middle-range theories. Hopefully, the current study will build upon and integrate with these.

Theoretical Sensitivity and Insight

Glaser and Strauss develop a related conception called "theoretical sensitivity." In effect, they mean the observer can see important theoretical issues in his data. This seems to be closely related to what Malinowski called "foreshadowed problems" which we've discussed elsewhere (Smith and Geoffrey, 1968). Some of their initial discussion seems to border on the mystical as they describe how one ferrets ideas out of data. Early in the book the basic position they take, in contrast to Malinowski's concern for knowing considerable amounts of theory before one goes into the field, is to consider what groups or subgroups one moves to, toward, and through as one samples theoretically. The major criteria for choosing groups are what they call "theoretical purpose and relevance." In general, they contrast that to being constrained by structural circumstances of research and constrained by "preplanned, routinized, arbitrary criteria based on the existing structural limits of everyday group boundaries [p. 48]." Rather neatly, they describe the verificational trap that often occurs when one finds more interesting ideas than

the ones a study started with, and yet one feels constrained not to shift the focus of the study. In a sense they let their data push and pull them about. In effect, they have a kind of flexibility in getting off of problems that turn sterile and moving into issues that they have not anticipated.

Much of their discussion related to theoretical sensitivity becomes clearer later in their amplified discussion of insight and theory development. If there is part of the text to which we would give our wholehearted endorsement, it is this concluding chapter entitled *Insight and Theory Development*. We particularly appreciate it since it formalizes and clarifies some of our own thoughts about insights as well as some of their earlier more ambiguous remarks on sensitivity. In our own field work we typically read into our notes thoughts that we have variously referred to as "interpretive asides," "summary observations and interpretations," or "observer comments." Much of this material can be subsumed under the more general rubric of "insights."

Their discussion focuses "on the researcher as a highly sensitized and systematic agent." The basic assumption is that "the root sources of all significant theorizing is the sensitive insights of the observer himself." Three corollaries are derived from this assumption. The first states: "the researcher can get--and cultivate--crucial insights not only during his research (and from his research) but from his own personal experiences prior to or outside it." Our own experiences confirm the truth of this statement. Our past experiences as teachers are constantly refined and sharpened as we observe classrooms in action; parallels and differences hitherto unnoticed become fresh insights as we observe student teachers now in an additional setting (Connor and Smith, 1967; Pohland and Gussner, 1968); some characteristics of the migrant whites that we observed in the Washington School study come more sharply into focus as we continue our work in Eastern Kentucky. One might go on and on.

A second corollary is that "such insights need not come from one's own experiences but can be taken from others." Glaser and Strauss identify as specific "others" interviewees, informants, and novelists who may not only provide insights unwittingly, but also intentionally. Again we concur. For example, one of our developing interests in the CAI project has been the nature of politics in Eastern Kentucky. Without any doubt, we learned more about the inside workings of county politics in an informal discussion session one evening with a number of workshop participants than we had up to that point. One of the men present related stories of sheriff campaigns, financial arrangements, and the local liquor distribution industry.

A third corollary pertains to obtaining insights from existing theory. This is frequently referred to as the literature search. Again, it is obviously a valuable source of insights. For example, we have just finished rereading Rogers' (1962) *Diffusion of Innovation*.

In the context of our CAI study, this is providing useful insights. Glaser and Strauss raise the issue of the temporal aspects of the literature search. While they give no definitive answer, they do give the warning that "to cover 'all' the literature before commencing research, increases the probability of brutally destroying one's potentialities as a theorist [p. 253]." Our experience suggests that continuous reading intermixed with intensive observation and data collection is probably the better solution.

In Glaser and Strauss's discussion of the development of theory from insights, their basic stance is that "an insight, whether borrowed or original, is of no use to the theorist unless he converts it from being simply an anecdote to being an element of theory." Three corollaries are drawn from this:

1. Insights cannot be fruitfully developed, and are even unlikely to occur, unless the theorist goes beyond public discussion about any given area [p. 254].

2. The theorist should . . . develop comparatively the implications of his personal insights regarding [existing theory] [p. 255].

3. The ambitious theorist should not only cultivate insights until his inquiry's close, he must actively exploit their implications [p. 256].

It seems to us that what Glaser and Strauss are saying is that insights as insights are valueless unless integrated into the framework of the developing substantive theory. To us, this makes eminently good sense. Our own approach has been the pursuit of antecedents and consequences of important phenomena. These we build into figural models. The models can be converted in the kind of axiomatic propositional theory suggested by Zetterburg (1965). Our most thorough development of this occurs in chapter one of Smith and Geoffrey (1968).

Temporal Aspects of Theoretical Sampling

The authors conclude their discussion of theoretical sampling with an analysis of temporal aspects of sampling. Their major point is that the specific nature of generating grounded theory, that is, the joint collection, coding, and analysis of data, requires a different time sequence than other methodologies. They comment briefly upon the need for "respites for reflection and analysis" of the data already collected; the need to pace the "alternating tempo of his collecting, coding and analyzing in order to get each task done in appropriate measure, in accordance with the stage of his research and theory development [p. 72]"; the temptation to overextend the research in order to "know everything"; the difficulties of anticipating the time needed to complete a study; the extensive and

indeterminate amount of time required to work one's self into the significant social systems to the point where information is freely obtained; and the time required to mine the contributing slices of data, *e.g.*, reading in related literature. To all this we chorus a hearty "Amen!" Our own experiences, while corroborating the above, suggest, however, another significant factor: the unanticipated contingencies. For example, in our current project we had anticipated completing the major portion of our data collection (field work) by early December. However, an array of problems developed which auger for extending the observational period about six months. This has consequences all of its own.

We would raise two further issues. Glaser and Strauss state, "collection of additional data can be a waste of time for categories already saturated or for categories not of core value to the theory [p. 73]." We have already stated our general position with regard to data collection. Here we only wish to reiterate that in our view "saturation" occurs only when the researcher has mined the data of all possible insights, and that this capacity is limited only by the richness of his theoretical background and creativity.

Secondly, and more seriously, the authors raise an ethical issue. In discussing the time-consuming aspects of data collection, they say that in the later stages of data collection a field worker "may obtain his data clandestinely in order to get it quickly, without explanations, or to be allowed to obtain it at all [p. 75]." We are not suggesting that the authors advocate this approach. We do note that data so obtained raises the issue of the right of privacy, may jeopardize the current as well as future research, and may be of dubious value since it can rarely be reported.

The Discovery-Verificational Continuum Reanalyzed

Introduction

Glaser and Strauss make another necessary distinction, the possible conflict between generating and verifying theory. In a sense, this is also the same distinction we have been making in accenting the former rather than the latter. Their treatment of the problems encountered by scholars who wish to develop theory in this "grounded" sense and the more traditional scholars interested in verification, has all of the emotional components that we have found in our own work. We are particularly concerned about making this work acceptable to our colleagues because of the problems encountered by students who wish to pursue this style of investigation and, in effect, break with the traditions ofthe last three or four decades within both educational psychology and educational sociology.

Our personal preferences are to move toward verification in settings where careful controls can be instituted, careful measures developed for significant concepts, and large enough samples of subjects can be obtained. The concept of teacher awareness, geneated in Smith and Geoffrey (1968), has received this attention in Smith and Kleine (1969).

In the social science field another major quarrel has existed regarding the importance of qualitative versus quantitative data. Depending on the issues under discussion within the methodology, verification of theory and discovery of theory, the qualitative-quantitative issue can become a "red herring." Because the generation of theory and the use of qualitative data tend to be correlated, they tend to be attacked or supported indiscriminately. Some of the variations on this theme, for instance, the work of Festinger, Riecken, and Schachter in their book *When Prophecy Fails* (1956), have been treated in our methodological discussions in *The Complexities of an Urban Classroom*. In that analysis we presented qualitative work in a verificational context as the atypical example of the usual use. However, their work does represent an important effort and does indicate possibilities in the use of qualitative data for hypothesis testing. We feel now as we stated then (Smith and Geoffrey, 1968), a number of events and problems that might have generated important theory were not raised and discussed by Festinger and his colleagues.

The Constant Comparative Method of Qualitative Analysis

Here Glaser and Strauss get to the heart of their approach to the discovery of grounded theory. Briefly, the constant comparative method is a technique of joint coding and analysis of data for the purpose of systematically generating theory. The key to the procedure is in the "explicit coding and analytic procedures" outlined in the following steps: "(1) comparing incidents applicable to each category, (2) integrating categories and their properties, (3) delimiting the theory, and (4) writing the theory [p. 105]." We shall return for a closer scrutiny of these steps shortly.

Glaser and Strauss contrast their constant comparative method with three other general approaches to analyzing qualitative data. The first is the conversion of "qualitative data into crudely quantifiable form" for the purpose of provisionally testing a hypothesis. Since testing hypotheses rather than generating theory is the thrust of this approach, the authors, like ourselves, are not particularly attracted to it. A second basic difference between this technique and the constant comparative method resides in the number of hypotheses and the level of generality. According to Glaser and Strauss, the former "is usually concerned with a few hypotheses couched at the same level of generality, while our method [constant comparative] is concerned with many hypotheses synthesized at different levels of

generality [p. 103]." Obviously, if one is interested primarily in testing, restrictions on scope are necessary.

The second general approach to the analysis of qualitative data is quite different. Here the emphasis is on generating theoretical ideas rather than testing them. The authors state:

> If the analyst wishes only to generate theoretical ideas . . . he cannot be confined to the practice of coding first and then analyzing the data since, in generating theory, he is constantly redesigning and reintegrating his theoretical notions as he reviews his materials. Analysis after the coding operation would not only unnecessarily delay and interfere with his purpose, but the explicit coding itself often seems an unnecessary, burdensome task. As a result, the analyst merely inspects his data for new properties of his theoretical categories, and writes memos on these properties [pp. 101-102].

It seems to us that this is a rather lucid description of our own basic methodology although we have not phrased it precisely in these terms (Smith and Geoffrey, 1968). Rarely have we explicitly coded our data. On the other hand, we are in the constant processes of inspection, "redesigning and reintegrating our theoretical notions." Glaser and Strauss go on to contrast this with their own "explicit and analytic procedures." They acknowledge the fact that

> this method does not supplant the skills and sensitivities required in generating theory. Rather, the constant comparative method is designed to aid the analyst, who possesses these abilities, in generating a theory that is integrated, consistent, plausible, close to the data--and at the same time is in a form clear enough to be readily, if only partially, operationalized for testing in quantitative research. Still dependent on the skills and sensitivities of the analyst, the constant comparative method is not designed (as methods of quantitative analysis are) to guarantee that two analysts working independently with the same data will achieve the same results; it is designed to allow, with discipline, for some of the vagueness and flexibility that aid the creative generation of theory [p. 103].

Somehow, again, we are unmoved by their argument. We would question whether all the time and effort expended in this process is worthwhile if the end result of theory so generated is "only partially operationalized" and replicability is in doubt. Furthermore, it seems to us that the purpose of any well-conceived methodology should

be to eliminate rather than allow for "vagueness" in generating theory.

Perhaps the basic difficulty we perceive in their method is that it tries to do too much, disclaimers not withstanding. Apparently Glaser and Strauss recognized this problem since they go on to state:

> By contrast [to method one], the constant comparative method cannot be used for both provisional testing and discovering theory: in theoretical sampling, the data collected are not extensive enough and, because of theoretical saturation, are not coded extensively enough to yield provisional tests, as they are in the first approach. They are coded only enough to generate, hence to suggest, theory [p. 103].

Yet it seems to us in the light of their previous statement, that this is precisely what their constant comparative analysis tries, in part, to do. Perhaps our own preference for method two lies in our more modest goals.

The authors go on to compare their constant comparative analysis with "analytic induction." Briefly, this is concerned with "generating and providing an integrated, limited, precise, universally applicable theory of causes accounting for a specific behavior [p. 104]." In effect, this is a combination of procedures one and two in that it "tests a limited number of hypotheses with *all* available data, consisting of mumbers of clearly defined and carefully selected cases of the phenomena" and generates theory through the constant "reformulation of hypotheses and redefinition of the phenomena" as a function of the occurrence of "negative cases." The basic differences between this approach and their own lie in the emphasis on testing and the restriction imposed by the analytic induction method to "causes" rather than the wider range of "conditions, consequences, dimensions, types, processes, etc."

Utilizing the Constant Comparative Method

We return at this point to a more careful analysis of the steps of the constant comparative method. Step one in the process, as noted previously, is to "compare incidents applicable to each category." Briefly, once the data are gathered, the analyst codes each incident into as many emergent or extant categories as possible. The process is refined in line with "the basic, defining rule": "*while coding an incident for a category, compare it with the previous incidents in the same and different groups coded in the same category* [p. 106]." This procedure is designed to start the analyst thinking about "the full range of types or continua of the category, its dimensions, the

conditions under which it is pronounced or minimized, its major consequences, its relation to other categories, and its other properties [p. 106]." With the exception of the explicit coding, we have found ourselves engaged in exactly this process. For example, in our CAI project "systems breakdown" quickly emerged as one of the basic categories. As specific incidents occurred, we found our major category quickly breaking down into relevant subcategories, *e.g.*, programming difficulties, technical difficulties, personnel problems, organizational problems, and the like. These in turn were further refined into subcategories, *e.g.*, technical difficulties broke down into line trouble, computer breakdown, terminal breakdown, and so forth. We found ourselves positing relationships between the various subcategories as well as between categories. Our preference has been to go to the antecedents-consequences route. We find that as we draw out the models, the interrelationships become increasingly clear.

Glaser and Strauss propose that two general types of categories emerge: those he has constructed himself, and those that have been abstracted from the language of the research situation. Our own experience confirms this. While we were able to generate a category such as "systems failure" ourselves, it was only in direct conversation that the broader category of "line trouble" emerged or the still more refined class of technical difficulties centered on "loading the machine."

Once the coding is well underway, the authors suggest that conflicts in coding are likely to arise. At this point rule two goes into effect: "*stop coding and record a memo on your ideas* [p. 107]." Two major reasons are advanced for doing so: (1) to tap the initial freshness of the idea, and (2) to relieve the analyst's conflicts. We have found this process most fruitful, although we have been more prone to categorize such memos as "summary observations and interpretations." On occasion these have been formalized into lengthier documents, both as working papers and publications. In all cases, we would suggest multiple copies to "cut and paste" as one reworks and incorporates these memos into the finished document.

Glaser and Strauss also discuss the advisability of talking over "theoretical notions" with one's research partners. Again, we have found this most helpful, as the authors point out, in obtaining richer insights from the data, comprehensiveness in scope, clarity, and resolution of conflict. A basic position we take is that each researcher brings to the task a set of unique as well as common skills and insights, largely as a result of his own experiences and training. We have found our "phantasizing at 20,000 feet aboard an airliner" most stimulating.

The second step in the process is "integrating categories and their properties." Essentially, this shifts the focus of attention from comparing incident with incident to comparing discreet incidents with properties of the category derived from previous incident to

incident comparison. Ultimately this procedure is designed to achieve a closer integration of properties internal to a given category and integrate diverse categories into a theoretical whole. This is essentially what we hope to accomplish as one goal of our CAI study--the integration of categories and their properties into a theory of innovation.

"Delimiting the theory" is the third step. Here, three factors are operating on two levels. The first is the discovery of higher order concepts which subsume a number of lower order concepts. This in turn leads to generalizations and the emergence of a formal rather than substantive theory. In addition, the reduction of terminology to higher order conceptualizations and generalizations leads to the fulfillment of two major theoretical requirements, *parsimony* and *scope*.

Secondly, delimiting the theory provides a check on the number and range of categories possible in any research effort. This is back to their earlier point of uncovering such a plethora of data and their interrelationships that the main thrust of the research becomes subordinated to secondary although personally intriguing issues. Our CAI project provides a good case in point. We are constantly tempted to move from the main issue into related but secondary issues such as school organization and the nature of politics in Eastern Kentucky, classroom games, the sociology of underdeveloped regions, institutional influences, and the like. In short, delimiting the problem imposes constraints on the researcher in the accomplishment of his main objective.

The third factor operating in delimitation is the concept of *theoretical saturation*. In addition to coping with the problems of existing categories and the continued necessity of coding within these domains, it suggests a procedure for dealing with new categories. In brief, the authors propose coding for a new category only from that point that the category emerged rather than tracing previously coded materials for prior evidence. We are skeptical of this position. It seems to us that if one is trying to uncover the antecedents, the better practice is to review the notes in toto. Conversely, Glaser and Strauss suggest that coding cease once a category is saturated. In essence we have done this same thing. For instance, we feel that our "systems breakdown" category is sufficiently saturated at this point in time to discontinue intense efforts to obtain this type of data.

A final economic argument is advanced at the close of the discussion on delimiting theory. Limitations on personnel, time, and financial resources are readily recognized by anyone engaged in research. This alone would be a necessary if not sufficient reason for limiting the theory to manageable proportions.

The final obvious step is "writing the theory." If one follows the Glaser-Strauss procedure, one has the basic materials ready for writing: coded data, a series of memos which easily become chapters, and a theory. Somehow this sounds all too easy. Our own plodding efforts to write lucid, accurate, and convincing prose suggest more "blood, sweat, and tears" than do the glib assertions of the authors. We find ourselves more in accord with their comment:

> When the researcher is convinced that his analytic framework forms a systematic substantive theory, that it is a reasonably accurate statement of the matters studied, and that it is couched in a form that others going into the same field could use--then he can publish his results with confidence [p. 113].

The concluding section of the chapter, *Properties of the Theory*, is essentially a summary with a few elaborations on topics previously discussed. As an example of elaboration, they introduce the term *diversity* as a more general term for the process of comparing incidents. More important is their discussion of the three key "properties" of grounded theory. First, they note that the constant comparative method is particularly suited to the generation of "developmental" rather than "static" theories. Since our work, too, is largely devoted to the study of "processes, change, organizations, positions, and social interaction," this has an immediate appeal to us. Secondly, they note that it is by nature inductive, and that "to make theoretical sense of so much diversity in his data, the analyst is forced to develop ideas on a level of generality higher in conceptual abstraction than the qualitative material being analyzed." We would argue that our own work constrains us into the same channels, *e.g.*, our miniature theory of personalized interaction (Smith and Geoffrey, 1968). Thirdly, they argue that the constant comparative method "can yield either discussional or propositional theory." While we are not sure that the term "discussional theory" is a particularly happy or precise choice of words since it leaves itself open for misinterpretation, we find that much of our own writing falls into this category. For Glaser and Strauss, propositional theory is little more than a reformulation of "discussional theory," differing only in style rather than content.

Summary

The two by two table that Glaser and Strauss suggest for the approaches to qualitative analysis seems too simple. Their table (p. 105) is included as Table 1.

Insert Table 1 about here

TABLE 1
Use of Approaches to Qualitative Analysis

Generating Theory	Provisional Testing of Theory: Yes	Provisional Testing of Theory: No
Yes	Combining inspection for hypotheses (2) along with coding for test, then analyzing data (1) Analytic induction (4)	Inspection for hypotheses (2) Constant comparative method (3)
No	Coding for test, then analyzing data (1)	Ethnographic description

Our own analysis would suggest that the mode containing dimensions of generating theory and provisional testing of theory might be stated better as a four-criterion entity. This could be sketched as Table 2.

Insert Table 2 about here

The four dimensions: descriptive narrative, generation of theory, verification of theory, and quantification have been placed on continua of emphasis from low through moderate to high. Five examples of field work have been utilized to reanalyze the discussion of Glaser and Strauss's constant comparative method. In so doing, major differences in approach have been isolated. Only Glaser and Strauss deemphasize the descriptive narrative. The generation of theory distributes the research styles. Festinger *et al.* comes in with an intensively developed theory; traditional anthropology is less interested in theory generation. In contrast Glaser and Strauss and Smith and Pohland strongly accent such efforts. The verification of theory is minimized by us (Smith and Pohland) except for the cumulative efforts of our research. Festinger *et al.* saw it as their principal target. Regarding quantification, Becker *et al.* are the only strong adherents. Glaser and Strauss fall midway on the continuum.

Conclusion: Applying Grounded Theory

Later in their discussion (Chapter 10) Glaser and Strauss return to a topic introduced in the first chapter: an analysis of the formal properties of grounded theory. Four are posited: fitness, understanding, generality, and control. Of these, fitness, *i.e.*, close correspondence to the realities of the substantive area, is conceived as the "underlying basis" of the theory. Predictably, such fit can, according to the authors, only be "induced" from diverse data. Given "fit," the authors contend that such theory "will make sense and be understandable to the people working in the substantive area [p. 239]." Understanding in turn "sharpens their sensitivity to the problems that they face and gives them an image of how they can potentially make matters better [p. 240]." Nevertheless, in choosing categories for their fitness and understandability, the theorist is cautioned to strike a balance between specificity and abstractness: "the categories should not be so abstract as to lose their sensitizing aspect, but yet must be abstract enough to make his theory a general guide to multi-conditional, ever-changing daily situations [p. 242]." Such is the concept of generality. This introduces the idea of theoretical flexibility. This allows the user to "bend, adjust or quickly reformulate" the theory as "he tries to keep up with and manage the situational realities that he wishes to improve." Thus, in effect, the

TABLE 2
Approaches to Field Work

	Emphasis		
	Low	Moderate	High
1. Descriptive Narrative	o		*o* x * *x*
2. Generation of Theory	*x o*	*	o x
3. Varification of Theory	x	o *x* *	*o*
4. Quantification of Data	*o* x *x*	o	*

Smith and Pohland	x
Glaser and Strauss	o
Becker *et al.*	*
Festinger *et al.*	*o*
Anthropology	*x*

user not only applies theory but becomes a theoretician in the grounded sense himself. This is clearly consistent with the assumption that theory is processual rather than static.

Control, the fourth property, has a number of aspects. In general, it suggests that the user "must be enabled to understand and analyze ongoing situational realities, to produce and predict change in them, and to predict and control consequences both for the object of change and for other parts of the total situation that will be affected [p. 245]." This implies two kinds of control variables: "controllable" variables and "access" variables. Controllable variables are those which give the change agent "a controllable theoretical foothold" in various situations. The authors note that these controls are frequently exercised by individuals sans theoretical guides. "Access" variables on the other hand are gatekeepers to the controllable variables. Furthermore, they "indicate how best to enter a situation in order to manage a controllable variable while not otherwise unduly disrupting the situation [p. 249]."

Our reactions to this discourse are somewhat mixed. We were struck first by the absence of the more formal syntax of theoretical properties. Terminology such as extensibility, causality, epistemic correlations, and so forth was conspicuous by its absence. Two thoughts come to mind: (1) are these formal requirements nonessential, and (2) are there substitutions offered? With regard to the latter, it seems to us, for example, that the use of "access" variables is no large improvement over the more common "intervening" variable. In short, we simply ask, *Is the formal language of science inappropriate when discussing grounded theory?* We think not.

Secondly, and more favorable, we heartily concur with the theoretical emphasis upon fit and understanding. We share the growing impression of many educators that the classroom teacher's rejection of most learning theory stems from a perceived lack of fit to say nothing about understanding, generalizability, and exercise of control. The step from experimental psychology to the exigencies of a classroom is enormous. Grounded theory attempts to close that gap. We would argue that we have tried to do precisely the same thing. In *The Complexities* we have dealt with the immediate and practical as well as the theoretical in such issues as assignment giving, the nature and purpose of patience and gentleness in teacher-pupil interaction, the impact of textbook teaching on aspects of classroom and school social structure and processes, and the like.

We are also sympathetic to the notion of the need for theoretical flexibility in diverse situational contexts. We wish, however, that Glaser and Strauss had raised some caution with regard to the extent of the pushing and pulling that could easily occur. It is entirely conceivable that the integrity of the theory itself might be lost by indiscriminate manipulation of key variables by untrained personnel.

Perhaps this is why they were so adamant in insisting earlier in the text that generating sociological theory is an activity "that *only* sociologists can do [p. 6]." We would broaden this to the generation of grounded educational theory by all professionals in education. Not only would we include the University "educationist," but also the practicing administrator and classroom teacher. While the organizational opportunities and constraints upon scholar-teachers have not been well analyzed in the literature, the conception of the scholar-teacher has been the focus of strong emphasis as an ideal type. Grounded theory and educational ethnography have important relevance for this conception.

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